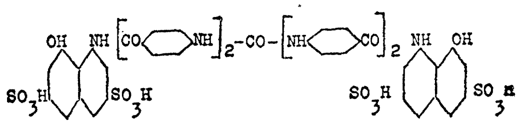
thalene-sulfonic acid and found that the most active trypanocidal agent was a dye prepared by the Bayer firm. Little notice was taken of this work, and the discovery of salvarsan diverted attention from the trypanocidal dyes to the organic arsenic compounds. The Bayer firm, however, continued to investigate the trypanocidal dyes and discovered that compounds of this type which were not dyes might still be active trypanocidal agents. They took out a large number of patents, and the type of compound to which the firm has paid special attention is represented by the following formula:



A number of substances of this type have been found to be very active trypanocides, and probably Bayer 205 is a derivative of this type. Bayer 205 is a white powder, freely soluble in water, forming a colorless solution, which can be sterilized. Animal experiments[2] have shown that it is an extraordinarily powerful trypanocidal agent, and that a single dose of it will produce immunity to trypanosomes for several weeks or even months. Mayer and Zeiss, for instance, found it cured infection with five different kinds of trypanosomes, that the ratio between the minimal lethal and minimal curative doses was as high as 167 to 1, and that a single dose of 3 mg. rendered a mouse immune to trypanosomes for three months. Recurrences were found to be extremely rare when infected mice were given a single curative dose of the drug.

The various workers have reported curative effects on trypanosomal infections in mice, rats, guinea-pigs, rabbits, dogs and horses. In England Wenyon[3] found that the drug was an extraordinarily effective trypanocidal agent. A brilliant success has been reported in a case of sleeping sickness.[4] [5] The case was of a year's standing, and had been treated unsuccessfully with arsenic, antimony and emetine. Four doses of "205," making a total of 3.5 grams, were given. A few hours after the first dose the fever disappeared, and a complete cure appears to have been produced, for four months later there were no signs of recurrence of the disease.

The drug therefore appears to be a trypanocidal remedy of the first importance, and the fact that a single dose confers prolonged immunity to trypanosomes suggests that it will be of the greatest value as a prophylactic. A commission of German doctors is now in Rhodesia testing the drug, and our knowledge as to its action in man will soon be much more extensive. The discovery of "205" promises to mark a great advance in tropical medicine, but it is a remarkable fact that England should be dependent on Germany for this advance in tropical medicine, for at present Germany has not a single colony, while England has the largest tropical empire in the world. It is not a position of which we have any reason to be proud, but its cause is simple. Germany appreciates the value of pharmacological research and we do not.—*The British Medical Journal.*

SCIENTIFIC BOOKS

Smell, Taste and Allied Senses in the Vertebrates. G. H. PARKER. Philadelphia and London, J. B. Lippincott Co., 1922, 192 pages, $2.50.

This little volume includes chapters on the Nature of Sense Organs, Anatomy of the Olfactory Organ, Physiology of Olfaction, Vomeronasal Organ or Organ of Jacobson, The Common Chemical Sense, Anatomy of the Gustatory Organ, Physiology of Gustation, and Interrelation of the Chemical Senses. In view of the author's long sustained interest in problems of integration of structure and function and his numerous successful experimental

2 Haendel and Joetten, *Bull. Instit. Pasteur,* 131, 19, 1921; Mayer and Zeiss, *ibid.,* 133, 19, 1921; Walther and Pfeiler, *ibid.,* 380, 19, 1921; Miessner and Berge, *ibid.,* 380, 19, 1921; Mayer, *ibid.,* 248, 20, 1922; Schuckmann, *ibid.,* 247, 20, 1922.

3 Wenyon, *British Medical Journal,* 1921, ii, 746.

4 Muhlens and Menk, *Muench. med. Woch.,* 1488, 46, 1921.

5 Yorke, *Ann. Trop. Med. and Paras.,* 479, 15, 1921.

attacks upon problems of this sort in the field of sense physiology, it is needless to say that his survey of the chemical senses is accurate, authoritative and judicious. So brief a summary naturally can make no claim to completeness, but it is a well balanced selection of topics of significance to students of physiology, psychology and allied sciences.

The more theoretical discussions, especially those centering about the genetic relationships of the receptors, naturally enter debatable territory. In summarizing his well-known observations on the organization of sponges, which "possess muscles but are devoid of nervous tissue," Dr. Parker reiterates (p. 21) his belief that in phylogeny differentiated muscles probably preceded nervous tissue. "So far as can be judged these [nervous] elements originated in connection with the previously differentiated muscle and as a special means of exciting it to contraction." This conclusion seems both unphysiological and unsupported by the facts.

That muscle should be differentiated in advance of the receptive apparatus through which it is activated seems *a priori* as improbable as that receptors should be developed in advance of the appropriate effectors. And Parker's own experiments strongly suggest that the so-called muscles of sponges are really excito-motor organs with lowered excitation threshold and that the excitation mechanism is elaborated within them parallel with the contractile mechanism rather than apart and subsequently. Ordinary protoplasm is, of course, both excitable and contractile, and in some unicellar forms (*e. g.*, Diplodinium, Euplotes, Paramecium and others recently described at the University of California) there are excito-motor masses of protoplasm in which these two functions are both highly developed and in various stages of separation physiologically and structurally. In sponges, as in other lowly multicellular forms, the excitation factor can not be regarded as lagging behind the contractile factor in the differentiation of the process and apparatus of reaction, even though the tissue involved may look more like muscle than like nerve.

In the concluding discussion (chapter 8) all receptors are arranged in three groups: (1) mechanicoreceptors (organs of touch, hearing, equilibration, and probably organs of muscle, tendon and joint sensitivity, lateral line organs of fishes and some others); (2) radioreceptors (organs of vision and temperature); (3) chemoreceptors (organs of smell, taste, general chemical sensitivity, and probably some others). This classification is natural and so far as it goes very satisfactory. But when the author adds (p. 180), "To ascertain into which of these three groups a receptor falls it is necessary to know how it is stimulated after which its classification is simple and immediate," one begins to question how far the simplicity and immediacy of the procedure really takes us.

Rays of the solar spectrum with wave length of say .0008 mm. falling upon the retina and upon the skin produce very different excitations. Both organs are by definition radioreceptors, but just "how it is stimulated" we do not know in either case, nor do we know how it comes about that, if sensation follows the excitation, it is of red in one case and warmth in the other.

Similarly, it has been shown by Parker that ethyl alcohol is an excitant of the organ of smell, the organ of taste and general mucous surfaces and by Carlson of the mucous lining of the stomach also. The threshold is different in each of these cases, the typical reactions are very characteristic in each case, and the sensations (if any) are likewise distinct. Now the fact that these four organs are all chemoreceptors, while important, is less significant biologically than any one of the other three criteria mentioned. In fact, the classification of receptors in terms of the effective stimulus has a very limited range of usefulness and the ultimate goal of our endeavors should be to add to the simple determination of the adequate stimulus of a sense organ the physiologically far more significant knowledge of the real nature of the excitation (that is, of the immediate protoplasmic response to the stimulus) and also a codified statement of the typical or physiologically "normal" more remote effects (reaction, sensation). We are at present very

far from the attainment of this ideal, for, as Parker says (p. 180), "The real difficulty lies in the fact that the numerous receptors that we now recognize have undergone varying degrees of differentiation and hence their mutual affinities are extremely diverse."

On the morphological side the difficulties are even greater, and the various attempts which have been made to determine which of the various anatomical patterns of end-organs are more primitive seem rather futile. Protoplasm in general seems to be sensitive to all of the three kinds of stimuli of Parker's classification and morphologically homologous organs seem capable of transgressing our artificial biological laws and "uniformities" in fashion most disquieting to the systematist. This is illustrated by Whitman's description (since confirmed by Hachloy) of the cutaneous sensillæ of leeches, which are tactile in function on the body but in the head become gradually metamorphosed into visual organs, and also by the way in which both olfactory and gustatory organs may serve on occasion as either interoceptors or exteroceptors, with characteristically different central connections and reaction types in the two cases.

No better summary of this phase of the matter can be given than the concluding sentences of Parker's book: "It is because of the repeated differentiations that characterize the evolution not only of the chemoreceptors but of the other groups of like organs that a classification of them or even a simple enumeration proves to be so unsatisfactory. For they are not unitary elements that can be counted like the fingers on the hand nor are they sufficiently co-ordinated to make classifications easy and natural. They are like the whole organism itself in that they exhibit that kind of diversity that characterizes evolutionary flux."

C. JUDSON HERRICK

SPECIAL ARTICLES

PERIGENESIS

I AM presenting here a short preliminary account of the results of a study of the division figures in *Tradescantia virginica* L.

With the methods used, the structure of the chromosome is that of an achromatic cylinder of jelly-like consistency as described by Vejdovsky (1912) in which the chromatin, however, is imbedded in the form of chromomeres rather than a spiral. These bodies are so distinct that in any one optical plane, they can be counted.

They are made up of flocculated chromatin particles which associate together in rather dense masses which are arranged inside of the periphery of the linin cylinder in such a manner that there results a central core of achromatic substance.

The relationships of the chromomeres one to the other seem to be somewhat variable although the chromosome often shows a quadripartite cross-section as figured by Merriman (1904), Bonnevie (1908), and by Nawaschin (1910).

The effect of fixing, imbedding, and staining this structure gives appearances which have doubtless led to the interpretation that it is longitudinally split.

The arrangement of the chromatin particles within the achromatic cylinder may be traced back, in the vegetative stages especially, to the earliest prophases and I do not find anywhere, either in the vegetative or reduction divisions, any further evidence of a longitudinal split so that for the reductions, I agree with Meves and others that there is no side by side pairing of the chromosomes in these stages.

I find as did Suessenguth (1921), in spite of the recent evidence in favor of a parallel conjugation, that the continuous prophase spireme is constricted into the chromosomes in end to end relationships.

Muller (1921), in discussing the work of Troland (1917) says, "If he is right, each different portion of the gene structure must—like a crystal—attract to itself from the protoplasm, materials of a similar kind thus moulding next to the original gene another structure of similar parts, identically arranged, which then become bound together to form another gene, a replica of the first."

From the phenomena in all metaphase figures, inasmuch as I find the separation is not by longitudinal division, I would limit the above quoted process to the stages beginning with